SATURDAY, JULY 31, 1880.

THE CEREBRAL FISSURES OF THE DOMESTIC CAT, *Felis domestica.*

BY BURT G. WILDER, M. D.

The discovery of the electrical excitability of certain cerebral convolutions renders more than ever desirable some common nomenclature of the folds themselves and of the fissures by which they are defined. For various reasons, some of which were stated by me in 1873 (11,219), the *fissures* should first be identified, and their names agreed upon.

Two notable contributions to fissural homologies and terminology have been recently published by Julius Krueg. An abstract of the paper on Herbivora was given by Horsley in *Nature* for January 23, 1879. The second paper, upon the fissures of the Proboscidea, Hyracoidea, and Carnivora, was published in January of the present year, but did not reach the libraries in this country until May. The larger part of the paper is devoted to the Carnivora, and will prove more useful, practically, than the remainder. After a general historical sketch Krueg discusses the manner of formation of the fissures, taking the cat as less subject to variation than the dog. He then enumerates the fissures, with brief characterizations, under three heads: "Grenzfurchen, Hauptfurchen, Nebenfurchen." The detailed account of the fissures is divided into "Canidæ, Felidæ, Hyænidæ und Protelidæ, Viverridæ, Mustelidæ, Procyonidæ, Ursidæ, Phocidæ und Otaridæ." A separate historical sketch is given with each section, and four of the five folding plates of excellent outline figures are devoted to the carnivoral fissures.

In fulfillment of a purpose announced in 1873 (11,229), I have nearly ready for publication a somewhat extended paper upon the Gross Anatomy of the Brain of the Domestic Cat. The conclusions which I had reached respecting the nature, relations and nomenclature of the fissures accord in most respects with those of Krueg. In the hope that his paper may incite others to take up this branch of comparative anatomy, I desire, upon the present occasion, to point out the improvements which Krueg has made upon his predecessors, and at the same time to suggest some amendments to his views.

Krueg does not state whether the brain figured by him is intended as a type, or is merely selected from among the 12 adult brains which he examined. The following figures represent what seems to me to be a comprehensive type of the fissural pattern of the domestic cat, based upon at least 200 specimens, mostly prepared by myself.[1]

EXPLANATION OF THE FIGURES.

The figures are enlarged about two diameters. Fig. 1, the lateral aspect of the hemisphere and lobus

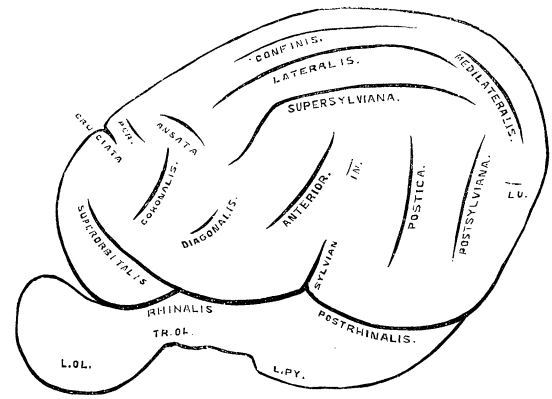

Fig. 1.

olfactorius; Fig. 2, the mesial aspect of the same, but as if viewed more from the cephalic region so as to expose the whole of the strongly curved fissura hippocampalis; hence the figure is somewhat foreshortened.

The constant fissures are shown as dark lines, the inconstant fissures as lighter lines. The f. olfactoria could not be shown upon these figures; it is a shallow groove upon the cephalic end of the hemisphere, and the lobus olfactorius rests in it.

The following abbreviations designate fissures: Ge.—Genualis; Ro.—Rostralis; Pmr.—Postmarginalis; Pr.—Postrhinalis; Pcr.—Postcruciata; Lu.—Lunata; In.—Intermedia. The name is placed above the fissure only in the case of the f. callosalis.

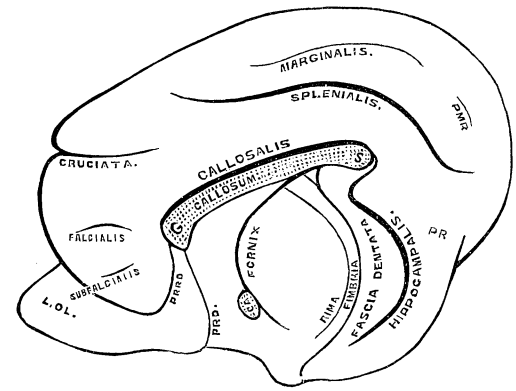

Fig. 2.

All of the names designate fissures, excepting the following names or abbreviations which refer to parts of the brain structure: Fornix; callosum—the corpus callosum; c. a.—the commissura anterior; rima—"the great transverse fissure;" fimbria—the corpus fimbriatum or taenia hippocampi; fascia dentata; S.—the splenium or caudal end of the callosum; G.—the genu or cephalic end; l. ol.—the lobus olfactorius; tr. ol.—the tractus olfactorius; l. py.—the lobus pyriformis.

The figures are diagrammatic, especially with reference to the structures represented upon the mesial surface; for instance the fornix is shown as a simple line.

[1] In the Museum of Comparative Zoology at Cambridge, Mass., is a series of 42 cats' brains, more than half of which are young or fœtal, forming part of a collection to illustrate the Neurology and Embryology of Domesticated Animals made by me for the late Professor Agassiz. In the anatomical laboratory of Cornell University each student prepares, draws and dissects two or more brains.

The lines represent what may be called the *fissural integers*, and only those junctions are shown which, so far as I know, are constant in the cat. These are of the rhinal with the postrhinal, and of the sylvian with the point of their union; of the superorbital with the rhinal; of the callosal with the hippocampal, and with the preradical when it exists.

The following junctions I have never observed: Of the splenial with the postrhinal; of the splenial with the cruciate, which Guillot has seen once, and Krueg twice. Neither have I seen the union of the anterior and posterior fissures to form the "first or lowest arched fissure" of the Canidae. On the contrary, as stated by Krueg (2,613), and by myself (11,229), this union sometimes fails with domestic dogs; hence, in this as in many other respects, the cat presents less tendency to vary.

The following junctions are common: Of the diagonal with the anterior; of the postsylvian with the supersylvian; of the medilateral with the lunate, and with the lateral or the confinis; of the marginal with the post marginal; and of the ansate with the lateral or coronal or both.

A junction is usually marked by a less depth of the compound fissure at that point, constituting a concealed "transition convolution" or "pli de passage," which may be seen by separating the sides or by slicing off the cortex.

The fissura ansata is represented by me as a slightly curved line nearly at right angles with the lateral and coronal, and rather nearer the former. It is true, as stated by Krueg, that the apparent form of the fissure is usually triradiate; but the variations are so great that no single figure would fairly represent them all, and in two brains I have found the condition of things shown in the figure. This fissure demands fuller investigation, especially with reference to its representation in the human brain.

The sylvian fissure in the cat does not present the complexity observed by Krueg in some dogs and in Ungulata, and the "Insula" is not distinguishable.

My paper will contain a *synonymy* of the fissures of the cat's brain, with full references to the page and figure upon which a fissure is named or represented. In some cases there are 25 entries under a single head, and I trust the lists may aid others in the identification of the fissures as described by different authors.

ABRIDGED SYNONYMY OF THE CEREBRAL FISSURES OF THE DOMESTIC CAT.

Abbrev.	*Flower*, 1869. *Leuret*, 1839. *Huxley*, 1861, 1872.	*Owen*, 1868.	*Wilder*, 1873.	*Krueg*, 1880.	*Adopted in the present paper.*	
An				Ansata	Ansata	An.
A.		Ant. branch of ectosylvian	Ant. upright of ectosylvian.	Anterior	Anterior	A.
C.		Callosal			Callosalis	C.
Cf.		Part of medilateral.	Part of medilateral	Confinis	Confinis	Cf.
Co.		Coronal	Coronal	Coronalis	Coronalis	Co.
Cr	Crucial	Frontal	Frontal	Cruciata	Cruciata	Cr.
Di.				Diagonalis	Diagonalis	Di.
Fl.		Part of falcial		Genualis	Falcialis	Fl.
H.	Hippocampal. Dentate, H., 1861	Hippocampal		Hippocampi	Hippocampalis.	H.
L.		Lateral	Lateral	Lateralis	Lateralis	L.
Lu.					Lunata	Lu.
Ml.		Part of medilateral	Part of medilateral	Medilateralis	Medilateralis	Ml.
Mr.		Marginal		Suprasplenialis	Marginalis	Mr.
Ol.			Ectorhinal	Olfactoria	Olfactoria	Ol.
Pcr.				Postcruciata	Postcruciata	Pcr.
P.		Post. branch of ectosylvian	Post. upright of ectosylvian.	Postica	Postica	P.
Pmr.				Postsplenialis	Postmarginalis	Pmr.
Prd.					Postradicalis	Prd.
Pr.		Part of ectorhinal	Part of rhinal	Rhinalis post	Postrhinalis	Pr.
Ps.		Postsylvian	Part of supersylvian	Suprasylvii post	Postsylvviana	Ps.
Prrd.					Preradicalis	Prrd.
R.		Part of ectorhinal	Part of rhinal	Rhinalis	Rhinalis	R.
Sfl.		Part of falcial		Rostralis	Subfalcialis	Sfl.
Sp.	Callosomarginalis, (Fl. & H)	Supercallosal		Splenialis	Splenialis	Sp.
So.	Supraorbital, (Fl)		Presylvian	Præsylvii	Superorbitalis	So.
Ss.		Supersylvian	Postsylvian & supersylvian.	Suprasylvii	Supersylviana	Ss.
S.	Sylvian	Sylvian	Sylvian	Sylvii	Sylviana	S.
In.					Intermedia	In.

The foregoing is an abridgement of this synonomy limited to writers who have made special additions to the technical nomenclature, and excluding those who have employed phrases or vernacular names, or who have adopted the names of other writers in purely physiological papers. Notwithstanding the importance of the contributions of Flower, Huxley and Leuret, the technical names employed by them are so few that they may be given in a single column. It is due to Krueg to state that several of the names now given had been already used in his paper on the Ungulata, in which he included a diagram of a dog's brain.

The principles of anatomical nomenclature are hardly identical with those of taxonomy, but it seems right that priority should prevail excepting when the name implies an incorrect or doubtful homology, or is practically very objectionable. Hence, Owen's "Postsylvian" should not be displaced by Krueg's "Suprasylvi posterior," or his "marginal," by "su-

prasplenialis." Likewise, Flower's "supraorbital" has priority of my "presylvian," which Krueg has adopted. On the other hand, Krueg's "anterior" and "postica" are so much more usable than previous names as to be worthy of acceptance, especially as they may be regarded as abbreviations of the phrases by which Owen and myself designated the fissures in question. "Splenialis" also is to be preferred to "supercallosal" or "calloso-marginalis," so long as the human homologue of the fissure is uncertain. If *marginalis* be retained, *postmarginalis* will be better than "postsplenialis."

I am particularly gratified to find that Krueg admits as fissural integers the *ansata* and the *diagonalis;* the former I had intended to call *transversa*, and the latter *intermedia*, but Krueg's names must be retained. We agree also in regarding Owen's "medilateral" as composed of two fissures, which Krueg terms "medilateral" and "confinis." I had intended to leave Owen's name attached to the fissure which is really mesiad of the lateral, and to call the curved division *lunata*. I still think this would have been preferable; but as it is, the name *lunata* may be given to what would otherwise have been *sublunata*. I have applied the name *intermedia* to a fissure which Krueg mentions, but does not name.

Doubtless my readers, especially those who are especially interested in the physiological aspect of the subject, desire to learn the correspondence between the cat's fissures and those of monkeys and man. I hope that Krueg may shortly give us the benefit of his opinion. Meantime, I am obliged to admit my doubts with regard to all excepting the callosal, hippocampal, and olfactory; for the Sylvian is not yet fully understood. I believe that for a long time to come the most useful work will be done upon nearly related forms, and that each fissure should be monographed with respect to its constant and variable characters, its connections, its relations to internal structures or to more primary fissures, and especially its manner of formation.

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A PLEA FOR THE METRIC SYSTEM IN MICROSCOPY.

By R. H. Ward, M. D., Pres. Am. Soc. of Micr.

One of the most important questions, theoretical and practical combined, which is now fairly before the microscopical world and still in an unsettled state, is that of gaining definiteness and uniformity in *micrometry*. In this field emergencies have arisen during the past year which have compelled me to take considerable responsibility, as well as to perform a large amount of work, trusting that the generous approval of my colleagues would accept and ratify what seemed at the time, and what seems now, most consistent with the interests of science and the dignity of this body. It will be remembered that a year ago, just at the close of our Indianapolis meeting, resolutions were offered favoring the metric system for micrometry, and the one hundredth of a millimeter as the unit to be employed, inviting foreign co-operation, and accepting an offer of standard micrometers from Prof. William A. Rogers, of the Astronomical Observatory of Harvard University. None of these points, save the last, were new or unconsidered. They had been studied at leisure for years by many members who were present. The metric system had been adopted by all the world except Russia, England and the United States; and its universal adoption was, as a rule, earnestly desired and favored by the educated and scientific classes. It had been adopted, or recommended, after mature deliberation, by the National Academy of Sciences, the American Metrological Society, the American Association for the Advancement of Science, by the American Society of Civil Engineers, the United States Coast Survey, the United States Marine Hospital Service, the American Medical Association, the Congress of Ophthalmologists, and by the largest State and local Medical Societies and by leading Medical Schools and Journals, by numerous Boards of Education, College Faculties and local Scientific Societies, and by experts in various branches of science and art. On the other hand the resolutions contained some minor faults, mostly in matters of taste or tact, which could have been easily remedied by reference to a committee. But there was no time for reference or for adequate discussion, and rather than discourage their object by failure or postponement, they were adopted and referred to the local Societies for consideration. They were passed unanimously, at a small session, it is true, but by the same vote which established this society and authorized its meeting here to-day. As too often happens, their incidental faults attracted more attention than their really scientific object. The unit proposed was evidently too long for integers and too short for fractions, and unlikely to receive a single approval either at home or abroad; the proposal of international action, though its object was universally approved, was in a form not likely to accomplish that object; and the liberal offer of Prof. Rogers was wholly misunderstood and perverted, until it took the form of the preposterous statement that it was proposed to make Prof. Rogers' micrometers standard as distinguished from those of other (!) makers, not the least amusing of all the blunders and absurdities of this precious statement being that of bringing the association, in any manner, by trade rivalry or mercenary considerations in relation with the work of one of our most generous scientists who has freely shared with the public every result of his labors, while pursuing them at an extravagant cost, and without a thought of pecuniary return. It soon became evident that an organized treatment of the subject was required to secure a proper and unprejudiced discussion of the objects of the resolutions. Feeling much responsibility as the presiding officer of this Society, and of one of the oldest of the local Societies, but having no authority to appoint an evidently necessary committee that should represent not only this Society but also sections of the country not yet named upon our rolls, I brought the subject before our local Association, and we invited all the Societies that could be reached to join with us in the selection of a National Committee for the consideration of this subject. The response from the large and active Societies, and from distinguished individuals, was a cordial and almost unanimous approval. Many of the Societies nominated to the committee members distinguished as specialists in this branch of microscopy; both Societies and eminent scientists contributed valuable opinions upon all the points at issue; and a large committee was organized which will, at a proper time, tender a report of progress to this Society. And while speaking of this committee, I will take the liberty of saying that it would be a pleasure to me, and I doubt not to all of us on this side of the lakes, if our friends from Toronto or Montreal, or any other points in the Dominion which may be represented here, would nominate members, and thus make it an American instead of a national body. To prevent confusion or misapplication of the practical suggestions which follow, and which naturally belong to this time and place, it is necessary to anticipate the report of the committee so far as to say that it will recommend to this Society to rescind its approval of the one-hundredth of a millimeter as the unit of micrometry, and to so modify the forms of the other resolutions as to leave the important questions of accurate measurement and convenient and scientific nomenclature in a favorable form for the attainment of valuable results.

Whether this Society, as such, shall continue to be known as actively interested in this reform, it is for you to say; though I sincerely hope that the members will unanimously agree with me in judging that it ought to do all that its influence, without dictation, can do in this direction. But I for one do not deem the decisions of Societies or other corporate bodies decisive and final. I am not much elated by their approval, or discouraged by their opposition. I have an average amount of respect for the motives but not for the efficiency of legislation. In State, in Church, in Science, it is possible and easy to carry out laws about in proportion as they are unnecessary. People who do not need government are easily governed. Persons who appreciate authenticated micrometers will use them if they can, with or without the approval of societies; and those who do not desire them will be about as little controlled by official decisions. While the encouragement and support of Societies and officials are welcome and valuable as far as it extends, I have more faith in the power of individual influence, and to that I look for an example which is able to settle this question beyond appeal.

In our micrometry we have the anomaly of a system of work capable of a precision almost, if not quite, unknown elsewhere to human art, for what other wholly artificial procedure possesses a demonstrated limit of accuracy inside of the 1-300,000th of an inch, and yet, until now, we have made no reasonable effort to free ourselves from avoidable errors known to be many times larger than that amount. While coal at $4.00 a ton and muslin at six cents a yard are, or at least pretend to be, measured with apparatus that has been carefully verified by standards of known quality, we have been measuring spaces almost infinitessimally small by standards of only commercial quality and possessed of manifest and uncorrected errors. This fact is too suggestive of the days when micrometers consisted of grains of sand and clippings of wire; with the odds against us that we know how to do better. Arrange your microscope so that it will magnify 3,000 or 4,000 times, making the one-thousandth of an inch on the stage seem three or four inches long through the lenses, then arrange an ocular micrometer so that the magnified one-thousandth of an inch shall be covered by, for instance, one hundred divisions of the ocular scale, and finally ascertain exactly how many of the one-thousandths of an inch on that or any other plate will be similarly measured by precisely the same one hundred divisions above it. Judging from my experience and from that of others who have tried the experiment, you will probably find a perfectly measurable discrepancy between the different spaces of the same name; so that even your own measurements, with the same apparatus, will not be comparable with each other unless, as is often done, you select some one average space as a basis of comparison, and are careful to use only that. Now we are trying to ascertain which of these various spaces is the correct one; or if not one is right, then to obtain one that shall be; or if that can not be done, at least to determine a known error from which we can compute definite results. This is not a question of makers, or dealers, or trade interests in any form, but of unmixed and independent science. We are attempting to procure a standard because we need it, and we hope for the cordial assistance of microscopists of really

scientific spirit in the difficult work of attaining it, and in the almost equally important task of bringing it into general and respected use. I call this a standard for convenience, and not in a strict or ultimate sense. Strictly it is only an authenticated copy of a standard, or a portion of a standard, namely, of the world's standard meter or standard yard; and hence, the importance, not fully shared by the original metre itself, of corresponding perfectly with its theoretical length.

The adoption of the metric system has a formal sound, and its difficulties have been, to say the least, well represented. But, to the extent of its use in micrometry, it really presents no difficulties and many advantages. The value of the millimeter and its decimals must be made familiar to the mind for other purposes, even for the understanding of exclusively English literature, and to use it for our measurements and statements will merely assist to keep it fresh in mind. The English system, or rather tradition, presents no pair of units so convenient for the microscopist as the millimeter for large objects and the 1-100oth millimeter for small ones. For the purposes of most people, for use in micrometry alone, it is sufficient to remember that the millimeter is about one twenty-fifth of an inch, and surely this is no great intellectual task. Nor would it waste a large portion of a lifetime to learn the whole series from the meter down, remembering that, in round numbers, the meter is a yard, with three or four inches to spare, the decimeter one-tenth of that 40 inches, or 4 inches, the centimeter one-hundredth of that 40 inches, or 4-10ths of an inch, and the millimeter one-thousandth of that 40 inches, or 4-100ths, or 1-25th of an inch. The real difficulty lies, I believe, not in memorizing the value of the few new units required, but in the awkward and useless habit of stopping to translate every item from the new unit to an old one. Any one can add a few new words to his vocabulary, a few new units to his tables, without harm. The telephone and the phonograph have brought no disaster along with their new double Greek names. An educated person can learn in an hour all the new terms, values and proportions of the whole metric system, with its interesting and suggestive relations; and the time would be well spent though he never used the system again. But I know by experience that he can also use it again, easily. When you once learn by a little practice to think in the new units the same as in the old, the apprehended difficulties vanish unaccountably and can scarcely be brought to mind. If asked to estimate the width of this room in yards, only a child unfamiliar as yet with the practical use of measures would say to himself, "It seems to be about 90 feet, which would be 30 yards." You would rather look at the wall to see how many times longer than a yard it is. So if you will take a metric rule, learn well how the millimeter looks, and its dek, the centimeter, and learn to use it in measuring and estimating the size of suitable objects, such as insects or flowers, you will find it as easy to think in millimeters as in lines, inches, feet, or yards, to say nothing of the comfort of knowing that you are in no danger of being lost between several kinds of the same name.

Aside from the selfish though sufficient motive of our convenience, I hope we shall practically adopt the metric system, because we can thus contribute a trifle of influence toward its general introduction. It seems plain enough now that our country made a serious mistake in not adopting it at first; and I am satisfied that it is still best for us to use it, notwithstanding the greatly increased difficulties in our way. It is not questioned that this is the best system ever tried or proposed, and the only one that can possibly come into general use; it is not denied that it would simplify education, and substitute order and intelligible relations for the confusion of our present metrology; it possesses as many points of convenient relationship to our old system as could reasonably be expected in any new one; it is admitted to be excellently adapted to all scientific work; it has been satisfactory to mechanics and manufacturers who have actually used it; it has been gradually and completely introduced into large shops using costly tools and machinery, without serious expense, and to the satisfaction of the managers. Almost without exception its friends are those who have used it, and objections to it come from those who have not. You hear less of the evils it has caused than of those it would cause. Furthermore, it offers us a carefully elaborated scheme of international co-operation, which we have but to adopt in order to place ourselves in harmony with the rest; the metric system is all international. It also unifies almost entirely the records made by persons adopting different units, since a statement of size will be practically the same to the eye and to the ear, and will require no formal mathematical reduction, whether in centimeters, millimeters, or in decimals of a millimeter. Fortunately we have all tried the experiment for ourselves, in one department, and know what some of the objections are worth. Our system of currency is precisely like the metric series of weights and measures; and is marked essentially by the same evils and benefits. Who now believes that having adopted a currency incompatible with the English system has caused us a hundredth part of the trouble it has saved, notwithstanding that it lacked the advantage of putting us in harmony with the rest of the world? Who now feels cut off from the past because of the change, or regrets the loss of the pounds and shillings so long as he has dollars enough and of the right kind (it is not easy to satisfy everybody about that)? Who has found the poor oppressed and the laboring classes annoyed by the system we adopted? Who has yet incurred a burdensome expense in hiring accountants skilled in a foreign and to us obsolete nomenclature to compute from the records of the past how many pounds, shillings and pence our grandmothers paid for their bonnets, or our grandfathers for their ships or their farms? The truth is the new system is so much better for our present purposes that we are glad to use it as soon as we fairly know how; and I believe that the same would be true of the whole metric system. We do not undervalue the records of the past, with their elaborate computations, and tables, and surveys; but few of the people of to-day come in contact with these directly, and those few could afford the extra trouble for the sake of the far greater interests involved. It is not scholars to whom learning in an unfamiliar form is a terror; they will spend lifetimes in working over such lore, merely for the pleasure of the work. And so much of it as is required for use in the daily life of the illiterate world is consistantly modified, and modernized, and adapted, by specialists of various kinds who inherit the progress of the past but adopt the fashions of the present.

To adapt a homely phrase which has remarkably vindicated itself, in another field in recent history, the proper way to introduce the metric system is to introduce it; not to decide why others should use it, but to use it ourselves. Nor need we wait to be certain of the feasibility of securing its universal use. It may be profitably used in science though not accepted in trade. The chemists have adopted it fully and with satisfaction in their work, in their teachings and in their books; the physicians are adopting it in different parts of the country, and the microscopists may well enjoy its facilities whether others do so or not.—(*Inaugural Address, Buffalo*, 1879.)

EXPERIENCE, says the *Electrician*, has shown that the life of a submarine telegraph cable is from ten to twelve years If a cable breaks in deep water after it is ten years of age it cannot be lifted for repairs, as it will break of its own weight; and cable companies are compelled to put aside a large reserve fund in order that they may be prepared to replace their cables every ten years. The action of the sea-water eats the iron wire completely away, and it crumbles to dust, while the core of the cable may be perfect. The breakages of cables are very costly, and it is a very difficult matter to repair them, in comparison with a land line. A ship has to be chartered at an expense of $500 a day for two or three weeks in fixing the locality, and in avoiding rough weather, as cables can only be repaired in the calmest seasons. One break alone in the Direct Company's cable cost them £20,000 to repair, and the last chance left to the company was to make an agreement with the Anglo-American, so that they should be protected and have the use of that company's line when their own was stopped.

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PROFESSOR LEIDY'S "FRESH WATER RHIZOPODS OF NORTH AMERICA."

Dr. Leidy is acknowledged to be the highest authority on the subject treated in his great work, "Fresh Water Rhizopods of North America;" a criticism of the book becomes, therefore, a work of supererogation, and we reserve to ourselves the more pleasing task of pointing out its many beauties and particularly its importance as one of the most valuable contributions to the literature of microscopic forms of life.

Published by the Department of the Interior of the United States Government, and forming volume twelve of the "Report of the United States Geological Survey of the Territories" in charge of Professor F. V. Hayden, it is produced in a sumptuous form which no private publisher would have dared to imitate.

Dr. Leidy's Report covers about three hundred folio pages, illustrated by forty-eight full sized plates, printed in colors in the highest style of lithography.

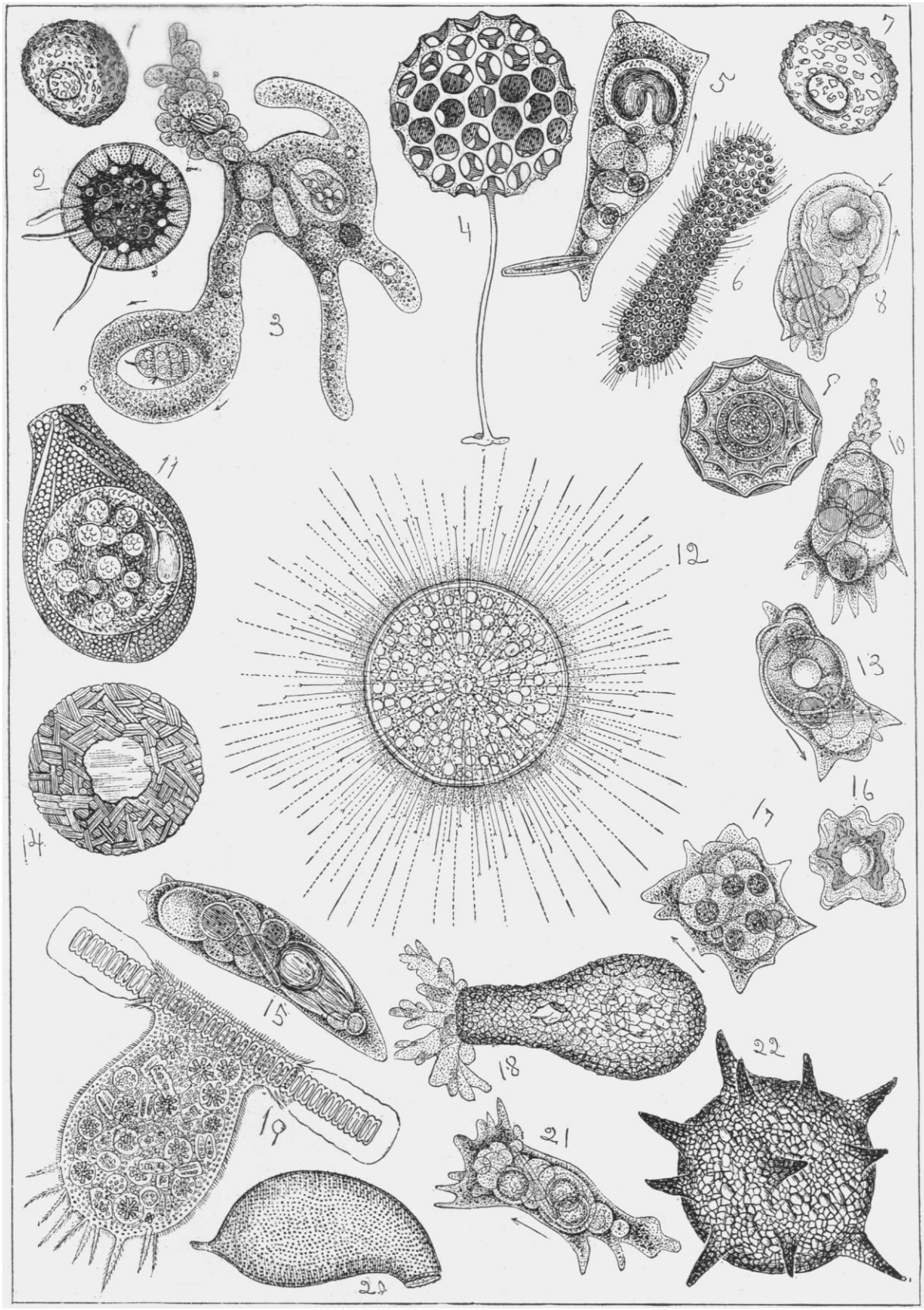
It may be a superfluous question to most of our readers, but as Dr. Leidy himself inquires in the first page of his work, "What are Rhizods?" In reply he says, "Rhizopoda are the simplest and lowest forms of animal life, constituting the first class of the Protozoa. They derive their name from the Greek word *rhiza*, a root, and *pous*, a foot. They are mostly microscopic beings, although sometimes sufficiently large to appear as conspicuous objects." We may add that the essential characters are the gelatinous structureless bodies, and the locomotive organs consisting of variable retractile root like processes (pseudo-poda or false feet).

Their minuteness is compensated for by their multitude and wide-world distribution; essentially aquatic they occur wherever there is moisture; the search for them may be commenced in the crevices of the stones at your door step, and may be continued in every marsh, pool, ditch, pond, lake, sea and ocean, and from the greatest depths of the latter to the snow lines of mountains.

The particular Rhizopods which form the subject of the book now under consideration, are those found in fresh water only, and Dr. Leidy expressly states that his attention, during the four years engaged, was directed more to the discovery and determination of the various forms occurring in the United States, than to the elaboration of details of structure, habits, modes of development, and other matters pertaining to their history.

Although it is professedly an illustrated catalogue of the fresh water Rhizopoda of North America, we find most interesting and valuable contributions to their life history which makes us regret that time and opportunity did not permit Dr. Leidy to extend his observations in this direction, for we know how exhaustive such a treatise would have been from his hands.

Instead of writing a discursive essay upon Dr. Leidy's book, already done by many brillant writers, which, with a work so purely technical, seems the least profitable method of treating the subject, we propose to take our readers through the book, acting the part of a friendly guide, trusting by the aid of twenty-two illustrations we have reproduced for this purpose, not only to do justice to the work in question, but to convey to those who have not



Drawn and Engraved for "Science."

studied the subject, a fair idea of the nature and form of Rhizopods.

In figure (3) we have a representation of the Amœba proteus, the most simple and wonderful of Rhizopodic forms—it is, to all appearance, a structureless gelatinous mass. As the *International Review* states (we fear somewhat rashly) it is without organs; they are certainly not permanently visible, and so far the most intelligent research has not discovered them; but the Amœba has the power of extemporizing such organs as are necessary for its existence, and has the means of reproducing its species. In the drawing, the pseudopoda, or false feet, are seen extended, and the animal is sketched while in the act of capturing an infusorial, by the act of joining the points of two of these pseudopoda.

In the Dinamœba mirabilis, figure (19), we find an advance made in structure, and a more specific form; the interior may be noticed to be full of desmids on which the animal feeds, and its posterior is widely expanded, so as to embrace a cord of Didymoprium.

In figure (18), Difflugia pyriformis, our Rhizopod is now found with a case or shell formed of irregular particles of quartz sand.

The empty shell of another of the same species, D. lobostoma, is shown at figure (14). The shell is composed of rectangular and oval plates, with dotted intervals.

Another of the Difflugia, D. Corona, at figure (22), still with a case formed of the same material, but of a somewhat different form, and having eleven spines; on the reverse side, the mouth would have been seen armed with teeth.

In figures (1) and (7), D. Constricta, we have further examples. In the original drawing they are colored a light redish-brown, whereas the previous examples were colored a blackish tint.

We now approach a beautiful design in form, the Nebela collaris, figure (11), the sarcode being contracted in the shape of a ball.

The design shown in figures (2) and (9), Arcella vulgaris, is still more beautiful and decidedly approaching the delicate patterns seen on the silicious valves of diatoms. The first in the original drawing is colorless, the second a light-brown, the sarcode being encysted.

In figure (20) is seen the empty shell of Cyphoderia ampulla, having a form somewhat similar to the Marine Rhizopods; the spur-like process may be noticed.

Clathrulina elegans, figure (4), is a very beautiful and remarkable form of Rhizopod, having a yellow colored lattice shell, enclosing sarcode in two balls, and supported by a stem of the same color. In other specimens Dr. Leidy shows the sarcode balls emitting numerous rays; this appears to be omitted in the present drawing.

At figure (6), Diplophrys Archeri, is a form of a different class taken from the swamp water on the mountains of Pennsylvania. Dr. Leidy describes them as composed of multitudes of minute globular individuals aggregated in masses, which in their movement causes the whole to undergo a change of shape. The corpuscle seen in each transparent body should be colored a bright cherry-red, to imitate the original drawing.

Acanthocystis chætophora, represented at figure (12), is very similar in form to the Actinophrys sol, or Sun animalcule, of the text books. The body is a finely granular protoplasm, invested with numerous delicate, silicious rays, implanted by minute basal disks; there are also numerous soft rays, like those of the Actinophrys, but distinguished from the silicious rays by the former ending in a simple pointed or furcate extremity.

We have now carried our readers briefly through Dr. Leidy's book, and have shown the beauty and variety of the forms studied. The interest attached to such investigations is obvious, and they are within the reach of every student. Considerable work has still to be done in this direction, and however great may be that done by Dr. Leidy, we feel sure that he would be the first to admit that so far the ground has been but broken.

To our mind it seems most important to concentrate future work on the genus Amœba, for therein lies the nucleus of the most valuable discoveries, which may even be of the highest importance to the human race. What is required in this direction is the constant and continuous observation of a single individual of the species, so as to arrive at its life history. We did hope that Dr. Leidy, in his present work, would have added to our knowledge on this point; but he frankly admits that neither himself nor others profess much knowledge relating to the reproduction of the Amœba.

It is, however, a question with us, if Dr. Leidy has not missed such an opportunity during his recent investigations.

At page 49 of this work on Rhizopods we notice Dr. Leidy describes what he calls an act of cannibalism, when he saw an individual (Amœba proteus) swallow and digest one of another species (Amœba verrucosa). We would ask, was this really an ingestive or a sexual act? Dr. Leidy takes the former view. With great respect for his

experience, we suggest that it may have been the latter, and we do so on the following ground.

In 1873 the Rev. W. H. Dallinger, F. R. M. S.,

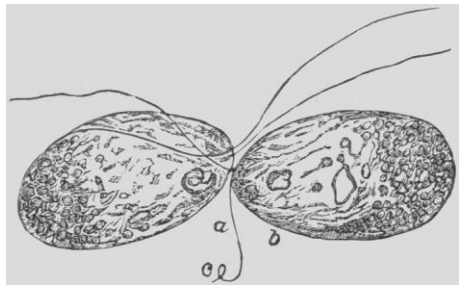


FIG. 1.

undertook, with Dr. J. Drysdale, to make a continuous microscopical investigation into the life history of a certain species of Monas. For this purpose they constructed apparatus which prevented the evaporation of the water, and the conditions were made perfect, to keep the specimen in a living state for a considerable time under the microscope. They then commenced to watch the monads in turns continuously, keeping the object in view without a break in observations for many days. During this close study of these monads, Messrs. Dallinger and Drysdale also observed an act so similar to that noticed by Dr. Leidy, that we will produce both statements for comparison.

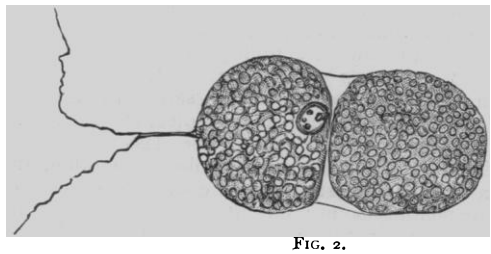


FIG. 2.

Messrs. Dallinger and Drysdale found the modes of re-production with the monads to be equally as varied as that known to exist with the Amœba, but there was one method which particularly attracted their attention.

Two monads at times met and touched their anterior ends, swimming freely together (figure 1), their bodies *then melted into each other*, (figure 2); it then became a single oblong mass, the line of juncture slowly disappearing, when after from six to 24 hours, it became rounded, (figure 3); at length the edges gave way, and myriads of minute points poured out, which were watched until they developed into perfect monads.

In figures Nos. (5, 8, 10, 13, 15, 16, 17, 21)* of our illustrations may be seen in the drawings, from Dr. Leidy's work, of what he observed of the mingling of two Amœbæ; he first speaks of their mutual approach, followed by an "embrace," when the jointed ends of the pseudopods *fused together*, and one sank deep into the body of the other, and eventually assumed the appearance of a sphere; further internal breaking up was then noticed, but later observation was not made. This is to be regretted as it seems quite probable that as "*the melting into each other*" was proved in the case of the monads to be a sexual act, that the *fusion*, when noticed by Dr. Leidy in the Amœba, may have been of the same character.

The necessity for the continuous mode of study

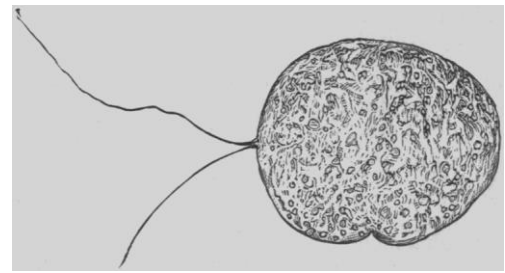


FIG. 3.

of such forms becomes more apparent every day, especially in the case of the Amœba, for in the very next paragraph to that just quoted, Dr. Leidy states "in my studies of Amœba proteus I have not been so fortunate as to trace its history from an early period, nor to discover its modes of reproduction."

There are other points to which we would refer, but having already exceeded our limit of space, we can only conclude by extending our thanks to Dr. Leidy for this his latest and most valuable contribution to SCIENCE, and to those who had the foresight and judgment to direct its publication.

*Figures (5, 8, 10, 13, 15, 16, 17, 21) illustrate the series of changes observed in the swallowing and digestion of an *Amœba verrucosa* by an *A. proteus*. From Bristol Marsh, Pennsylvania, August 27, 1876, 500 diameters. Fig. (16). *Amœba verrucosa*, comparatively quiescent with central contractile vesicle. Figs. (5, 8, 10, 13, 15, 17, 21). Successive changes in shape and relative position of *A. proteus* during the act of swallowing and digesting the former. Fig. (21). *A. proteus* approaching the *A. verrucosa* with anterior short diverging pseudopods. Fig. (8). The *A. proteus* embracing closely the *A. verrucosa* by a pair of digitate pseudopods, the points of contact of which being marked by the left hand arrow. Fig. (13). The *A. verrucosa* swallowed and forming a large sphere within the *A. proteus*. Fig. (10). The *A. verrucosa*, within the latter, has assumed an oval form, and is contained within a vacuole. The central contractile vesicle, which until now had remained persistent, had become less distinct. Fig. (15). The *A. verrucosa* has assumed a pyriform shape within a large elliptical vacuole and its contractile vesicle disappeared. Fig. (5). The *A. proteus* in the act of discharging a diatom, while the *A. verrucosa* has become doubled on itself. Fig. (17). The remains of the *A. verrucosa* seen as five granular balls within the *A. proteus*. Later these balls disappeared and their material appeared to be diffused among the granular contents of the *A. proteus*.

THE FOURTH STATE OF MATTER.

A REFUTATION.

Translated for "SCIENCE" by Gustave Glaser, Phil. D.

It may interest the readers of "SCIENCE" to know the opinion held in Germany respecting those phenomena which led Mr. W. Crookes to believe he had discovered a fourth state of matter. For this purpose we have translated and abridged an article by Dr. J. Puluj, the well-known scientist of Vienna, published in the "*Chemiker Zeitung:*"

According to Mr. Puluj, the beautiful experiments of W. Hittorf published in 1869, under the title "Electrical Conductibility of Gases," have received too little attention from our scientists, it may be, on account of the modest title. The scientific labors of Goldstein, and some interesting researches of Reitinger and Urbanitzky have met with the same fate. W. Crookes, the renowned English chemist, to whom the writings of the above-named gentlemen were evidently unknown, made similar experiments, the results of which did not differ essentially from those of Mr. Hittorf. His conclusions were, however, entirely new; he declared that his experiments proved a fourth state of matter.

The conception was daring, still more daring the hopes which he and his friends based upon the discovery of "radiant matter." The cause of these high expectations is the following: When an electrical inductive current is led through a molten glass tube in which the air is attenuated to $\frac{1}{1000}$ of its density, there appears on the negative pole a blue (glimmering) light, which is separated by a dark space from the cluster of light at the positive pole. If a greater attenuation takes place, the cluster of light disappears and the glimmering light floats over the whole tube, while at the same time, next the electrode a second dark space appears which becomes greater with the greater attenuation. If the attenuation still further increases, the dark space fills the whole tube and the glass walls shine in a brilliant, green, phosphorescent light. Mr. Crookes now believes that this phenomenon of phosphorescence comes from the remaining gas, which at this high state of attenuation has passed into an ultra-gaseous state, a "fourth state of matter."

But these phenomena are very different at a higher pressure. Direct measurements have shown that the phosphorescence does not appear at the millionth attenuation, and that the thirty thousandth attenuation is sufficient to produce it. Besides, this attenuated gas retains its characteristic properties, which could not be the case if by this attenuation it became dissolved into the original molecules which form, as Mr. Crookes says, the basis of all.

That the physical properties of this remaining matter are not changed, but remain in strict accordance with the kinetic theory of gases, also proves that we have no new state, but simply a gaseous state of matter. For example, the above-mentioned phenomena, in experimenting with the lighter gases, are visible at a lower altenuation than in experimenting with the heavier gases. The supposition of the renowned chemist, Dumas, that our elements are only chemical combinations of higher order, and complicated aggregates of primitive molecules, has, undoubtedly, much probability about it, but even the strongest electrical currents, and the highest temperatures, have not been able to produce this final dissolution of the elements, therefore it is not likely that a high attenuation can.

Dr. Puluj's experiments go to show that Mr. Crookes' so-called radiant matter "consists of negative electric particles," which are torn off from the negative electrode and hurled away with immense rapidity. These electrode particles form a very beautiful metallic mirror on the glass walls. [Aluminum particles are the only ones which form no metallic deposits. This may be accounted for by their chemical constitution.] The conduction of the current, therefore, is effected by the convection of the electrode particles, in which static electricity is accumulated. We have here a case of molecular electric convection, analogous to that observed by Mr. Rowland in his experiments. This gentleman has demonstrated that when a movable horizontally placed metal ring, charged with static, positive or negative, electricity is made to rotate around a vertical axis, it will divert a magnetic needle suspended above it, in the same manner, as if an electric positive or negative current were to move in the same direction with, or in an opposite direction from, the rotation. These experiments of Rowland lead to the inference that an infinitely small electrical globe, in our case an electrode particle, will have a similar influence upon a magnet. As long as the globe and magnet are at rest, it is to be expected that no alternate effect will appear, but that this will be produced as soon as the little globe is put into violent motion. Because the electrode particles are negative electric, they represent a positive electric current, which moves in an opposite direction from the former. The electrode particles in motion are, therefore, real elements of an electric current, and are subjected to the law of Laplace. Their deviation takes place according to the following simple law: If we imagine that a plane is placed through the direction of the motion of the electrode-particle and through the north pole of the magnet, and suppose that a man is lying upon this electrode particle in the direction of the motion, and looking towards the north pole, then the electrode-particle will be diverted towards the left hand of this man, vertical to the imagined plane. This simple law gives a sufficient explanation for all the phenomena which a magnet produces in the radiant electrode-matter, and which were observed by Mr. Crookes as well as by Mr. Reitinger and Urbanitzky. It proves that the glimmering light at the negative pole is not a "magnetic" light, but the consequence of a molecular electrical convection, and it justifies the supposition that an electrified current or vapor which is led through a tube will deviate the magnetic needle in the same manner as an electrical current going through a telegraph wire.

The law of the indestructibility of force has already solved many problems which puzzled the scientist of earlier centuries. According to the same law, we must assume that when infinitely small projectiles of radiant electrode matter are hurled against the glass walls of the tube their motion is changed into molecular motion, and the glass walls are heated by the collisions, sometimes even to the melting point; but at a lower temperature the rays which are not very much concentrated only produce a phosphorescent light of the glass.

The extremely fine matter called ether, which fills all space and pierces all bodies, surrounds the molecules, as the atmosphere surrounds our globe. Each body and each molecule has in its normal state a certain quantity of this ether. When this quantity is greater than the normal quantity, the molecules, according to the "unitarian view" of elasticity, are positive electric; when it is smaller, they are negative electric.

Supposing now that a collision takes place between the negative electrode particles and the molecules of the glass walls of the tube, then the equilibrium will be restored at each point of collision and the molecules of the glass will lose their surplus of ether. At the same time a motion of the waves of ether will be observed, and this motion is felt by our optical nerves as phosphorescent light. Therefore the phosphorescence observed by W. Crookes is the result of the restoration of the ether-equilibrium and not of the heating of the glass, whose temperature during the appearance of this phenomenon is comparatively low.

At a lower degree of attenuation, the stream of electrode matter pushes back the attenuated gas, and this explains the dark space which appears in the tube. This dark space is analogous to the dark space in a gas flame, which is to be seen near the mouth of the gas tube, and is produced because the outstreaming gas pushes back the particles of air which, coming from an opposite direction, try to enter the tube.

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Another observation of Mr. Puluj also contradicts the conclusions of W. Crookes. Puluj has observed that, at a higher attenuation, the electrode is moving towards the aluminium side, *i. e.*, in opposite direction from that observed by W. Crookes. According to Mr. Crookes the cause of motion is a double one, the higher temperature of the electrode at the metallic side and the emission of electrode particles.

Both effects are opposite. At a lower attenuation the effect of the heat is greater, and the electrode moves in the direction of the wings of the radiometer, with the colder side ahead, at a higher attenuation, the effect of the emission of electrode particles is predominant. Radiant electrode matter and the electrode itself move in the *same* direction.

This remarkable discovery proves not only the incorrectness of Mr. Crookes' explanation, but is also in direct opposition to the principle of the preservation of the centre of gravity, which is made by Mr. Crookes the basis of his arguments.

The Vienna scientist draws from his observations the conclusion that the forces by which the electrode particles are torn off are not interior but *exterior* forces. When the electric current passes through the electrode, there is, according to his opinion, really a stream of extremely fine matter (ether) flowing, which not only tears off particles of the electrode, but also sets the whole electrode into motion.

This view seems to be a new proof of the unitarian hypothesis, which maintains that an electric current is nothing else but a current of ether.

Even if the number of scientists who follow the dualistic hypothesis of electricity is by far greater than that of the Unitarians, the view of the latter deserves at least our attention, especially when such men as Franklin, Secchi and Edlund approved it.

THE MAGNET IN MEDICINE

Translated for "SCIENCE" by Thos. B. Columbia.

Some recent researches made under the direction of Prof. Charot in his laboratory at the Salpêtrière have drawn attention anew to a therapeutic agent known for a long time, but to-day almost abandoned. We find, in fact, even in the works of the oldest authors, traces of attempts made by physicians to apply the magnet in the treatment of disease.[1] But the want of precise rules in its application and the appearance of mystery and of fancy which is attached to this kind of research explain the discredit into which this means of treatment has fallen.

We are indebted to Prof. Maggiorani for having undertaken, in about 1869, the restoration of magnetic therapeutics, by seeking to establish it upon rational and truly scientific principles.

It was in the train of the experiments undertaken by the Commission appointed by the Biological Society, of Paris, with the object of verifying the facts collected by M. Burq under the generic title of Metallotherapy,[2] that the first attempts toward the application of the magnet were made at the Salpêtrière. After the results obtained by the application of metals, it was natural to seek to clear up the singular phenomena by varying as much as possible the conditions of the experiment. In this way it was shown that the plates of the different metals were not the only agents capable of acting upon a certain class of diseases (neuroses, and particularly hysteria, organic affections of the cerebral nervous system). Similar results were attained with many physical agents: feeble currents, statical electricity, vibrations of sonorous bodies, differences of temperature, magnetized bars, electro-magnets, solenoids, etc. Very soon the magnetic bars were noticeable for the constancy of their action and facility of their use.

Magnets are, therefore, not endowed, from this point of view, with specific properties; they form part of a group of physical agents which, to different degrees, possess the same power of impressing the nervous system and of giving rise to biological phenomena; and although magnets are here particularly spoken of, it must not be forgotten that they are not the only ones concerned.

The status of the question has been clearly exposed by Dr. Vigoroux in the *Medical Annual* (1879). To this article I must refer those who wish to become acquainted with the *ensemble* of phenomena, which are included under the name *metalloscopic*. These studies, begun at the Salpêtrière, have given rise to active discussions. The facts announced have been confirmed, wholly or in part, in Germany by Müller of Grätz, Westphal, Vierordt, Schiff, Adamkiewicz of Berlin; Benedick of Vienna, Rumpf of Dusseldorf; in Italy, by Seppilli, Maragliani, and especially Maggiorani; in England, by Gamgee, Sigerson, H. Tuke; in France, outside of the work of the Commission, I will mention only the thesis of M. Aigre and the observations of MM. Dumontpallier, Vigouroux, Landouzy and Debove, who have verified the therapeutical action of the magnet. But the results obtained were sharply attacked on the other side of the Channel by Hughes, Carpenter and Noble, who attempted to explain them by "expectant attention." In a thesis read before the Faculty of Medicine of Paris in 1878, Mr. Oscar Jennings made himself the champion of the ideas expressed by these English writers.

As to what relates to the magnet itself we are going to show, summarily, the arguments upon which are based its physiological action and its therapeutical use.

The action of the magnet, among effects produced by other physical agents of which we have spoken (plates of different metals, electricity, vibrations of the diapason), presents itself in a more surprising way, and, indeed, in a way *à priori* prone to excite incredulity. The application is not direct. The magnet is not placed in contact with the skin of the subject upon whom the experiment is tried, as it is necessary to do with other metalic plates, its action being exerted at a distance. It is sufficient to influence the organism, and produce the same effects as other metals, to place the magnetized bar at a distance of one to two centimetres from the portion of the body upon which we wish to make an impression. All the experiments at the Salpêtrière have been made with these conditions. The effects produced in these cases were not attributed to the action of the metal, and belong properly to magnetism itself.

The magnet, let us say, acts in some way on the organism when in these special morbid conditions. Before speaking of the facts which prove peremptorily that this action exists, can we not, if not explain it, at least conceive of the possibility of such an effect. The action of

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physical forces upon biological phenomena has long since been admitted; who does not recognize the importance of heat, of light, and of electricity upon the vital manifestations? In medicine electricity, under its different forms, is daily employed in the treatment of a multitude of diseases. Why refuse to one physical force that which we accord to all the others? Why, if all physical agents are only varied forms of the same force, should they not all exert an action upon the organism in a measure different for each of them? And then why should not magnetism, which possesses in such a high degree this very singular property of influence at a distance, be able in physiological order to produce analogous effects?

If from conjectures we pass to the consideration of facts, we are forcibly convinced that this physiological action of magnetism at a distance does truly exist.

In physics, experiment leads to a, in some degree, tangible result; it is undeniable; it forces itself upon us. Bring the poles of a magnetized bar near to some iron filings and you have the conditions for the experiment; the iron is attracted and there is the result. Discussions may arise upon the theory, upon the interpretation of the fact, but the fact itself is always present. Furthermore, as it is easy to appreciate exactly all of the circumstances of the experiment, we are certain that with the same conditions given, we shall always obtain the same result. In a word, the experiment can be easily repeated. In physiology experiment is surrounded by the greatest difficulties, but the result is neither less significant nor less certain. As regards experiments made with the magnet, it can be shown that they fulfill all the conditions of certainty of physical experiments.

In the first place it is necessary that the application be well done; that is to say, that the magnet ought to be in good condition and properly placed. It is not necessary that the magnet should be very large, nor endowed with very energetic properties; it is sufficient if the magnetic force exist in an appreciable degree. The experiment has often been attempted with false magnets, that is to say, with bars or horseshoes of variable substance, zinc, copper, wood, etc., not possessing any magnetic action, but having all the appearances of true magnets. In these cases the experiment has always given negative results. Likewise, in making use of the electro-magnet, the action upon the organism takes place only when the established current gives to the soft iron its magnetic properties. It is necessary also that the magnet should be properly applied; the poles alone acting, the neutral portion should remain absolutely without effect. This is easily accomplished when using the magnet in the form of a horseshoe and by presenting it successively by its open and its closed side. A patient has his eyes bandaged; the magnet is applied to the region of the back, in such a manner that he shall have no knowledge of its position, and physiological phenomena always follow such application of the poles, and never follow the application at the neutral line.

But is it easy to verify the result obtained? How does the magnet work? How is its action demonstrated? Does it not depend, one will say, upon phenomena of sensibility which are purely subjective and appreciable with great difficulty to the experimenter, who is obliged to trust to the word of his patient? I will reply at once that it is sufficient to have assisted at a single one of the experiments at the Salpêtrière to be convinced that these phenomena, subjective it is true, can be easily rendered objective. A large needle made to pass unawares through the flesh of the subject whose eyes are kept carefully bandaged, shows in an absolutely objective way the profound anæsthesia which the parts have attained. But the phenomena of sensibility are not the only ones produced; the magnet has an influence upon temperature, as the thermometer distinctly shows.[1] It acts also on the *molilité* of the parts to which it is applied, provoking contractions of an intensity and of a duration which removes all suspicion of simulation. The physician must be a mere novice who could mistake a prolonged and voluntary contraction for a true contracture. Now, the magnet produces in certain cases true contractures. (*La Nature.*)

DR. P. RICHET.

(*To be continued.*)

[1]M. Broca presented to the Academy of Medicine, at the Session of February 7, a note from Dr. Henrot (of Reims) upon the action of the magnet in hemihypothermia. He states that the application of three magnets to the cold limb raised its temperature from 1.8° to 2.3° in twenty minutes, at the same time lowering the temperature of the normal member two-tenths of a degree.

NOTES AND QUERIES.

To the Editor of Science:

[1.] In the 3d. July number of SCIENCE, under "Notes and Queries," J. H. G. desires "reliable information concerning the Tuckahœ."

I will tell him what I know concerning it, in regular sequence with his questions:

1st. What is its geographical distribution?

I know, from North Carolina to Texas, along the gulf States—presumably elsewhere also.

2nd. What is the nature of its growth and production?

An underground fungus or root, growing under the surface like the truffle. Belongs to the gasteromycetous fungi, according to Lindley and Fries.

3d. Its former use and preparation?

Used as bread, by roasting in the ashes, both by Indians and negroes.

4th. In what soil found?

Sandy or loamy surface with sub-soil of clay.

5th. What authors have mentioned it?

Dr. McBride and F. P. Porcher of South Carolina; Clayton, Le Conte, M. J. Berkeley, Fries & Lindley.

6th. By what botanical name is it known?

Lycoperdon solidum.

7th. Has it any medicinal virtues?

I know of none.

It is considered very nutritious hence its name *tuckahœ*, which is Indian for *bread*, and is highly prized by the negroes, who eat it to this day.

There is a district of country in the eastern counties of North Carolina called Tuckahœ from the abundance of these subterraneous bodies.

Although the Tuckahœ has been placed among the fungi, yet there is considerable doubt as to its true position in the vegetable kingdom.

Analysis shows it to consist almost entirely of pectic acid, which would seem to remove it from the fungi, and yet the entire absence of vascular or cellular structure of bark, etc., would seem to remove it equally as far from the phænogams.

I hope these few *enseignments* may help J. H. G., and that he will favor us with the results of his further researches.

MRS. M. J. YOUNG,

HOUSTON, TEXAS, July 19th.

THE Rev. W. Cowell Brown, Wesleyan minister of Sheffield, has patented an invention which appears to be a simple and practical means of lessening the number of deaths by drowning. A chemical preparation is inserted in a portion of the coat, waistcoat or dress. It does not add to the weight, or in any way alter the appearance of the garment. The preparation is inserted between the lining and the cloth; in the case of a coat, it is placed on each side of the coat and up the back. The moment a man falls into the water the coat becomes inflated, and he cannot keep his head under the waves. The invention has already been thoroughly tested, and it is stated that it will sustain a person in the water as long as he can possibly endure the exposure, say forty-five or fifty hours.